LETTERS TO THE EDITOR

Regarding "Ultrasound-guided infraclavicular brachial plexus block enhances postoperative blood flow in arteriovenous fistulas"

We have read the interesting article about the infraclavicular brachial plexus block by Sahin, and colleagues.[1] This is a well-designed and analyzed study. We have a few questions in order to clarify some important points about the subject. The most important fact is the success rate. The authors reported a 93.4% success rate in their series. In a previous study of ours,[2] we had an 80.9% success rate, which was comparable to the literature.[3] Of the bupivacaine group in our study,[2] three patients of 16 required supplemental local analgesic, which is higher than the authors' study. How would the authors explain their high success rate? In our study, we analyzed the pulmonary functions of the interscalene block. The bupivacaine group had significant changes compared with baseline, and three patients had respiratory distress. Did the authors have similar complications in their series? The primary patency rates are significantly different in two groups in the Sahin and coleagues' study.[1] We previously analyzed 116 patients for primary arteriovenous fistula failures.[4] The primary failure rate was 14.3% in that analysis. It was found that primary failure cases had higher body mass indexes and higher rate of hepatitis C antibodies. The secondary failure cases had lower antithrombin III levels compared with cases with no secondary failures. Could the authors comment about the failures in their study group and the differences between the two groups in their study? We would like to thank the authors for their study and would like to know their comments on the subject.

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REFERENCES

1. Sahin L, Gul R, Mizrak A, Deniz H, Sahin M, Koruk S, et al. Ultrasound-guided infraclavicular brachial plexus block enhances postoperative blood flow in arteriovenous fistulas. J Vasc Surg 2011;54:749-53.
2. Altintas F, Gumus F, Kaya G, Mihmanli I, Kantarci F, Kaynak K, et al. Interscalene brachial plexus block with bupivacaine and ropivacaine in patients with chronic renal failure: diaphragmatic excursion and pulmonary function changes. Anesth Analg 2005;100:1166-71.
3. Schroeder LE, Horlocker TT, Schroeder DR. The efficacy of axillary block for surgical procedures about the elbow. Anesth Analg 1996;83:747-51.
4. Danis R, Ozmen S, Akin D, Batun S, Kahvecioglu S, Altintas A, et al. Thrombophilias and arteriovenous fistula dysfunction in maintenance hemodialysis. J Thromb Thrombolysis 2009;27:307-15.

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Reply

We would like to thank Drs Polat and Gumus for their interest in our study. First, we used infraclavicular brachial plexus block for arteriovenous fistula surgery because this approach is one of the most recommended regional techniques. In addition to this technique, use of ultrasound increases the success rate and quality of the block. Our success rate (93.4%) is similar to that in the literature.[1] Interscalene block is well suited for surgical procedures involving the shoulder, clavicula, proximal humerus, and shoulder joint, but incomplete blockage of the inferior trunk often results in insufficient analgesia in the ulnar distribution for forearm surgery. Thus, we can say that interscalene block is not suitable every time and may result in a lower success rate of block for forearm surgery, as in their previous study.

In the interscalene approach, there is a high risk of hemidiaphragmatic paralysis because phrenic nerve neighborhood with brachial plexus in that area. But there is not a similar risk in infraclavicular block, and the risk of pneumothorax is minimized by ultrasound. In conclusion, we have not encountered respiratory distress in any patient.

In our study, primary patency and primary failure rates (7% vs 17%) are significantly different between the two groups. Mouquet et al[2] reported that, after brachial plexus block, brachial artery diameter and blood flow, as well as arteriovenous fistula blood flow, increased compared with controls. The incorporation of various brachial plexus block techniques in arteriovenous fistula construction thus appears to contribute to vessel dilation and reduced vasospasm by sympathectomy-like effects and may improve the success of vascular access procedures by significantly increasing fistula blood flow. The use of a regional block, compared with other anesthetic techniques such as infiltration and general anesthesia, have been shown to result in higher patency rates and lower failure rates.[3]

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REFERENCES

1. Gürkan Y, Tekin M, Acar S, Solak M, Toker K. Is nerve stimulation needed during an ultrasound-guided lateral sagittal infraclavicular block? Acta Anaesthesiol Scand 2010;54:403-7.
2. Mouquet C, Bitker MO, Bailliart O, Rottembourg J, Clergue F, Montejo LS, et al. Anesthesia for creation of a forearm fistula in patients with endstage renal failure. Anesthesiology 1989;70:909-14.
3. Shemesh D, Olsha O, Orkin D, Raveh D, Goldin I, Reichenstein Y, et al. Sympathectomy-like effects of brachial plexus block in arteriovenous access surgery. Ultrasound Med Biol 2006;32:817-22.

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Regarding "Primary aortoenteric fistula following endovascular aortic repair due to type II endoleak"

We read with interest the article by McPhee et al.[1] The authors argue that the case is best characterized as a primary aortoenteric fistula (AEF) as a native aortic wall eroded into the duodenum from ongoing aneurysmal enlargement. We have recently published a very similar case in a patient without endoleaks but with evident sac expansion that was attributable to endotension.[2] Clinical presentation and surgical findings were very similar to those reported by McPhee et al.[1] We think that both cases are secondary and not primary fistulas because primary AEF is a spontaneous communication between an unoperated abdominal aneurysm and the duodenum. The patients reported on in the article have been operated on and have a prosthesis in the abdominal aorta, which is

the cause of their clinical problem, although the fistulas were between the third portion of the duodenum and the native aneurysm sac and the endograft was not in contact with the fistula. In fact, the management strategy was also very similar and includes explantation and extra-anatomic revascularization. On the other hand, as we concluded in our article, we completely agree that AEF must remain in the differential diagnosis of any patients who present with upper gastrointestinal bleeding after endovascular aneurysm repair.

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REFERENCES

1. McPhee JT, Soybel DI, Oram RK, Belkin M. Primary aortoenteric fistula following endovascular aortic repair due to type II endoleak. J Vasc Surg 2011;54:1164-6.
2. Fernández-Alonso L, Alegret J, Urtasun F, Jimenez Arribas JM, Atienza M, Centeno R, et al. Aortoenteric fistula after endovascular abdominal aortic aneurysm treatment with the original Gore Excluder endoprosthesis and Cook aortouniiliac converter for endotension. J Cardiovasc Surg (Torino) 2011;52:391-4.

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Reply

We thank the authors of the Letter to the Editor for their careful review of our recently published article. Admittedly, we also struggled with how best to characterize the aortoenteric fistula described in our report. We agree that the standard definition of primary aortoenteric fistula refers to the erosion of native aortic tissue into the gastrointestinal tract in the absence of aortic manipulation while secondary fistulae are those that erode after aortic intervention. After peer review of our article by way of the *Journal of Vascular Surgery* editorial staff, we agreed with our reviewers that our case was most similar to a primary aortoenteric fistula, as the endograft itself did not erode but rather the enlarging native aneurysm sac eroded due to a persistent type II endoleak. We agree that if the endograft itself had been the nidus of erosion, it would best be characterized as a secondary fistula. Nonetheless, the salient characteristic of both cases remains the importance of recognizing this clinical scenario to allow for appropriate intervention when it arises.

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